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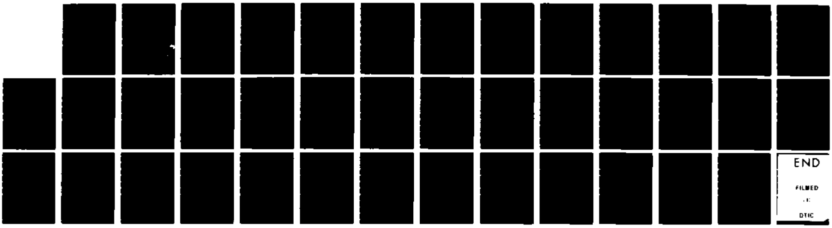

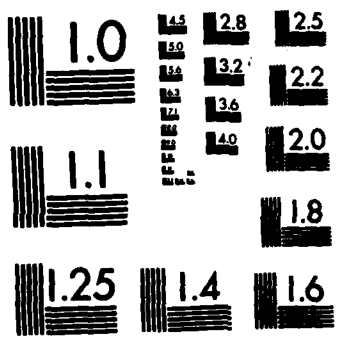


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# NONADDITIVITY IN INFERENCE JUDGMENTS

Lola L. Lopes

WHIPP # 16 November 1982

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It has been known for some time that subjects in Bayesian tasks produce data that look more like averages than like inferences. Shanteau ~~(1970, 1972)~~ suggested that the proper descriptive rule for the data is a weighted average. Wallsten ~~(1972)~~, however, pointed out that a constant weighted averaging rule such as Shanteau used is formally equivalent to the Bayesian rule in that both are qualitatively additive. In principle, however, averaging can be differentially weighted, in which case it becomes non-additive. This paper reports two experiments that test for additivity in

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an inference task. Although the group data were, on the whole, additive, there were numerous violations of additivity in the single subject data. These comprised three general categories: (1) violations that are interpretable as resulting from a serial adjustment process in which adjustments are toward the value of the new information presented, (2) violations that are interpretable as resulting from a serial adjustment process in which adjustments are Bayesian-like in their direction, and (3) violations that appeared to be systematic but that had no ready interpretation.

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## Nonadditivity in Inference Judgments

It has been known for some time that subjects in Bayesian inference tasks produce data that are more like averages than they are like inferences (Shanteau, 1970, 1972). In a recent series of papers (Lopes, 1981, 1982), I have argued that this may come about because subjects in these tasks integrate information serially via an "anchoring and adjustment" process (Einhorn & Hogarth, 1982; Lopes & Johnson, 1982; Lopes & Oden, 1980; Tversky & Kahneman, 1974; Wallsten, 1976b) in which "new" information is integrated into "old" judgments by adjusting the old value toward the new information, thus producing a final value that lies somewhere between the two.

Shanteau (1970) proposed that the inference process follows a constant weighted averaging rule in which the response to n samples is a weighted ($w_i$) average of the scale values ($s_i$) of the several sample events:

$$R_n = \sum_{i=0}^{n} w_i s_i$$

In Shanteau's model the weight terms, which are assumed to sum to unity, can vary as a function of the serial position in which a sample is presented or processed. They do not, however, vary as a function of the scale value of the sample information. The term $w_0 s_0$ is interpreted as an initial decision state.

Wallsten (1972) pointed out that the constant weighted averaging model is formally equivalent to the Bayesian model in that both models are additive in the sense of conjoint measurement (Krantz, Luce, Suppes, & Tversky, 1971). This is easiest to see by considering that Bayes' theorem is additive when a log odds transformation is applied to both sides.

Shanteau (1975) later attempted to bypass the formal equivalence between his averaging model and the Bayesian model by finding a qualitative distinction between the two that could be tested experimentally. He approached the problem by examining the effect of nondiagnostic samples on subjects' judgments. Shanteau reasoned that if subjects moved their judgments toward a neutral value after presentation of nondiagnostic information, this would provide critical evidence against the Bayesian model since, according to that model, nondiagnostic information should not affect the judgment.

Subjects' responses to nondiagnostic information confirmed Shanteau's expectations and, thus, seemed to lend support to the notion that subjects

in Bayesian tasks use some sort of averaging mechanism. Wallsten (1976a), however, pointed out that the averaging model correctly predicted subjects' responses only in cases in which the nondiagnostic information was presented following presentation of diagnostic information. For cases in which nondiagnostic information was presented first, the subjects' responses were better described by the Bayesian formulation.

Wallsten argued that a modified version of the Bayesian model could do better than either the averaging model or the simple Bayesian model. In his new model Wallsten outlined a stepwise procedure in which sequence information determines which of two additional parameters (which increase and decrease judgments selectively) is used to supplement the basic Bayesian formulation. This modified model was not intended to be taken seriously as a process account of performance in the Bayesian task (see Wallsten, 1976a, p. 330). However, Wallsten (1976b) noted that the modified model is a special case of an additive difference model which he proposed as a general description of human inference processes.

Wallsten's additive difference model is of interest because it was based on the same sort of anchoring and adjustment notion that has guided the present research. However, as I have argued elsewhere (Lopes & Johnson, 1982; Lopes & Oden, 1980), anchoring and adjustment can produce differentially weighted averaging in which the weight of a stimulus item can vary with factors other than serial position. This is important because differentially weighted averaging is distinguishable both from Shanteau's (1970) constant weighted averaging model and from Wallsten's (1976b) additive difference model by virtue of the fact that it is nonadditive.

For illustration, consider Figure 1. The rows and columns of the matrix give the absolute weights and the scale values for four hypothetical stimuli. The entires in the four cells show how these are combined into a weighted average. Notice that there are three different scale values (11, 13, and 15) and two different weights (10 and 1). The different scale values can be taken to represent different values of stimulus information and the different weights can be taken to represent different degrees of importance of the associated stimulus information. For example, the weight of a particular stimulus item might be affected by its reliability, its diagnosticity, or its relative novelty.

Note that the averaged values in the first column decrease from the first row to the second row. If additivity holds, the averaged values within the second column must also decrease from the first row to the second row since

| | WEIGHT = 10<br>SCALE VALUE = 11 | WEIGHT = 1<br>SCALE VALUE = 11 |
|---|---|---|
| WEIGHT = 10<br>SCALE VALUE = 13 | $\frac{10(11) + 10(13)}{10 + 10}$<br>$= 12.4$ | $\frac{1(11) + 10(13)}{1 + 10}$<br>$= 12.8$ |
| WEIGHT = 1<br>SCALE VALUE = 15 | $\frac{10(11) + 1(15)}{10 + 1}$<br>$= 11.4$ | $\frac{1(11) + 1(15)}{1 + 1}$<br>$= 13.0$ |

FIGURE 1. ILLUSTRATION OF HOW DIFFERENTIAL WEIGHTING WITHIN THE SAME FACTOR CAN PRODUCE VIOLATIONS OF ORDINAL INDEPENDENCE.

additivity requires ordinal independence, i.e., that the ordinal relationships of data in columns (and rows) must be the same for all columns (and rows). It is clear from the figure, however, that this does not occur. Instead, the values in the second column increase from the first row to the second row so that if these data were represented as a graph, the lines representing the two rows would actually cross one another.

It is important to note that the presence of differential weighting within factors allows violation of ordinal independence but does not require it. The reader may verify that if the respective weights in Figure 1 are set at 2:1 instead of 10:1, no crossover occurs; i.e., the data are ordinally additive. Thus, whether or not one gets violations of ordinal independence is, in part, a matter of the relative magnitudes of the particular weights and scale values that are assigned to the stimuli. In other words, finding violations of ordinal independence implies nonadditivity, but not finding such violations does not imply additivity.

It might seem that violations of ordinal independence such as those illustrated in Figure 1 would be unlikely to occur in psychological experiments. However, essentially identical violations of additivity were found previously (Lopes & Oden, 1980) in tests of an averaging model of similarity judgment. Thus, violations of additivity might occur in Bayesian tasks as well. In any case, it is reasonable and important to look for potential violations of additivity since this is the only way to distinguish mathematically between averaging processes such as the present serial adjustment model and other modified, but still additive, versions of the Bayesian model such as, e.g., Wallsten's additive difference model.

This paper reports the results of two preliminary studies aimed at testing for nonadditivity in a Bayesian task.

## Experiment 1

### Method

Experimental task. Subjects were asked to make judgments about the maintenance of hypothetical machines based on samples of parts produced by the machines. The judgment concerned whether or not a critical spring has broken inside the machine. Subjects were told that normal machines have a rejection rate of about 10 parts per 1000 (H10/1000) whereas machines with broken springs have a rejection rate of about 22 parts per 1000 (H22/1000).

Thus, the general task was to decide between alternative Bernoulli processes, one with $\underline{p} = .010$ and the other with $\underline{p} = .022$, where $\underline{p}$ is the probability of a rejected part.

Stimulus design. The stimuli for each trial were pairs of samples. The individual samples each contained numerical information about the estimated rejection rate for the machine (i.e., a digit between 10 and 22) and also information about the reliability of the sample. Subjects were told that samples could be either "spot checks" or "full checks." The instructions for this ran as follows:

> "Basically there are two different types of samples that are used. One type of sample is called a spot check. These are done very quickly by just taking a very small number of parts and using this to get a rough estimate of the machine's rejection rate. Obviously, spot checks give only ballpark data. The other type of sample is called a full check. These are very thorough checks of a large number of parts and are used to give good estimates of the machine's rejection rate."

The spot/full manipulation was intended to allow the weight of a sample to be varied independently of its content.

Experimental pairs were generated using the set of six 2 x 2 factorial designs shown in Figure 2. Matrices "Control L" and "Control H" were included as checks to be sure that in pairs having both a full check and a spot check, the spot check would not simply be ignored. Matrices "Critical L1", "Critical L2", "Critical H1", and "Critical H2" were the potentially nonadditive matrices in which stimuli having different weights appear in the same factor. Note that the values given in the matrices are intended only to show the pattern of interaction that would be expected if nonadditivity holds. The particular values were calculated using weights of 10 and 1, respectively, for full checks and spot checks, and setting the scale values of the sample equal to the number of rejects.

Procedure. Subjects were run individually in sessions that took about 30 minutes. At the beginning of a session subjects were brought into a sound proof booth and seated in front of a computer controlled video terminal. The stimuli were presented in the middle of the video screen with the samples one above the other, i.e.,

FULL CHECK 11
SPOT CHECK (15)

CONTROL L

| | FULL 11 | FULL 12 |
|---|---|---|
| SPOT 15 | 11.4 | 12.3 |
| SPOT 16 | 11.5 | 12.4 |

CONTROL H

| | FULL 21 | FULL 20 |
|---|---|---|
| SPOT 17 | 20.6 | 19.7 |
| SPOT 16 | 20.5 | 19.6 |

CRITICAL L1

| | FULL 11 | SPOT 11 |
|---|---|---|
| FULL 13 | 12.0 | 12.8 |
| SPOT 15 | 11.4 | 13.0 |

CRITICAL H1

| | FULL 21 | SPOT 21 |
|---|---|---|
| FULL 19 | 20.0 | 19.2 |
| SPOT 17 | 20.6 | 19.0 |

CRITICAL L2

| | FULL 12 | SPOT 12 |
|---|---|---|
| FULL 14 | 13.0 | 13.8 |
| SPOT 16 | 12.4 | 14.0 |

CRITICAL H2

| | FULL 20 | SPOT 20 |
|---|---|---|
| FULL 18 | 19.0 | 18.2 |
| SPOT 16 | 19.6 | 18.0 |

FIGURE 2. STIMULUS DESIGN FOR EXPERIMENT 1 WITH ILLUSTRATION OF NONADDITIVE PATTERN IN CRITICAL MATRICES.

Parentheses were put around values from spot checks as an additional reminder to the subject that these values were "rougher estimates than the one(s) from the full check(s)."

At the bottom of the video screen there was an unmarked response scale anchored at the right by the words "CERTAINLY FAULTY, 22/1000" and at the left by the words "CERTAINLY NORMAL, 10/1000." Just above the scale was a response arrow that could be controlled by movement of a joystick. Subjects responded to each stimulus pair by positioning the response arrow along the scale and then pushing a button to signal the computer to store the response. After this, an "X" appeared at the middle of the scale and the subject had to move the response arrow past the X and then push the button again. This initiated the next trial.

Altogether there were 10 trials for practice followed by two replications of the experimental pairs. These were interspersed with occasional "end anchors" that comprised pairs of samples that both strongly supported one or the other hypothesis. This gave a total of 82 pairs in all. Stimulus pairs within replication were ordered randomly, but with the restriction that no two pairs from a given matrix follow one another in the ordering. Stimuli within a pair were counterbalanced so that the row stimulus appeared in the upper position in one replication and in the lower position in the other replication.

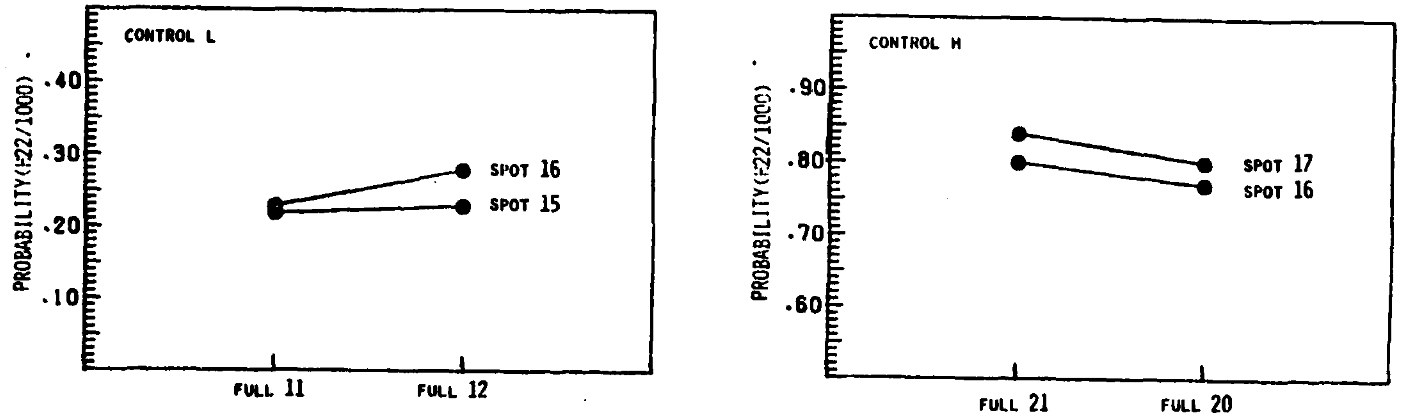
Subjects. The subjects were 41 student volunteers from the University of Wisconsin--Madison. They served for credit to be applied to their course grade in Introductory Psychology.

## Results and Discussion

The results of Experiment 1 are shown in Figure 3. Judgments have been scored so that 0 indicates complete confidence that the machine is normal (H10/1000) and 1 indicates complete confidence that the machine is broken (H22/1000).

The purpose of the control matrices was to check the possibility that subjects who are given information from both full and spot checks might simply ignore that from spot checks. 'identlv .is did not happen. In both matrices there were significant effects fo he ..w factor ($F(1,40) = 15.31$ and 20.75 for L and H, respectively) as well as for the column factor ($F(1,40) = 10.91$ and 18.2). Neither matrix had a significant interaction.

## CONTROL MATRICES




## CRITICAL MATRICES

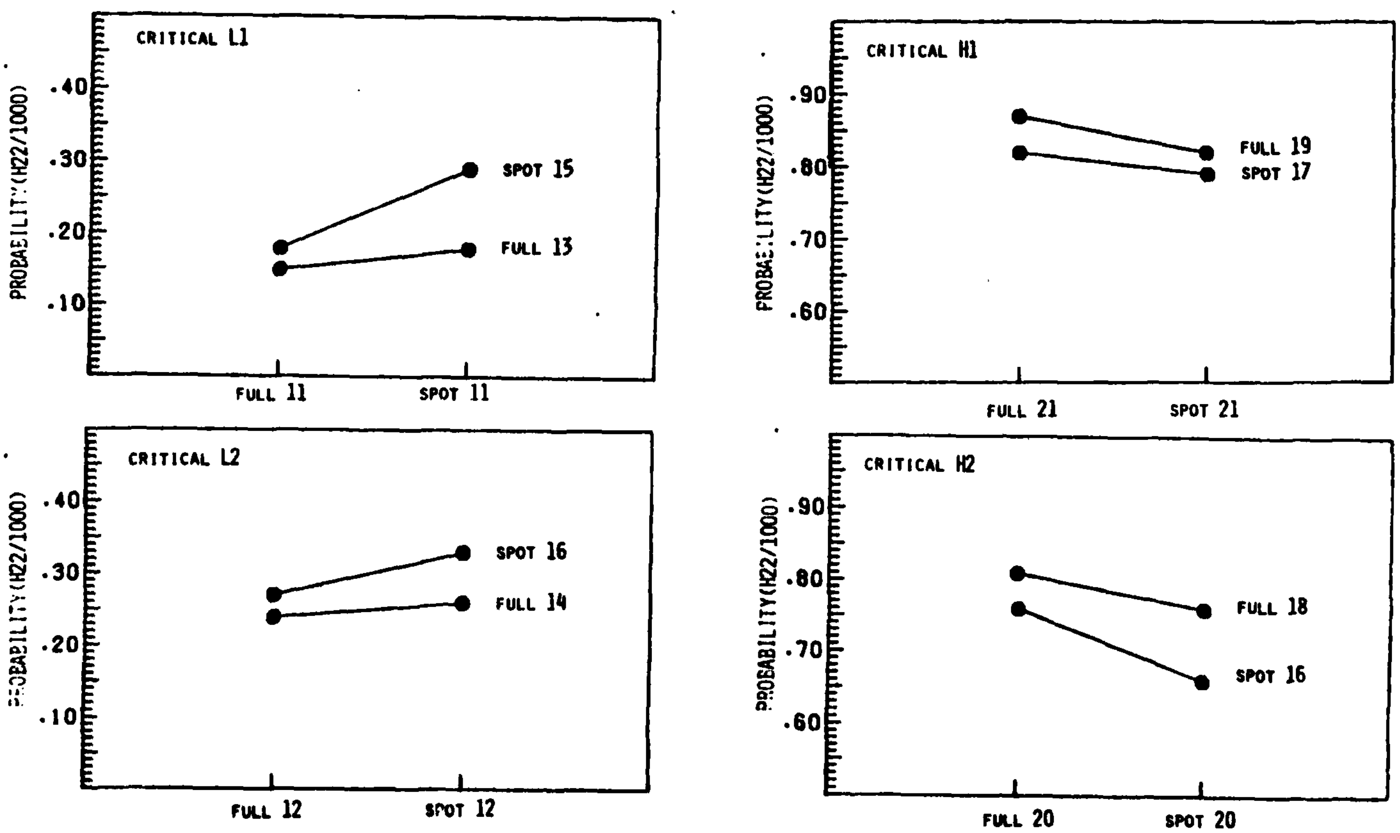



**Figure 3. Results from critical and control matrices of Experiment 1.**

The four critical matrices are those in which nonadditivity (i.e., crossovers) might have been expected to occur. It is evident from the graphs that, at least at the group level, the data are ordinally additive. Although three of the four matrices had significant interactions of the form generally expected ($F(1,40) = 34.85$, 4.46, and 5.60 for L1, L2, and H2, respectively), the curves for the spot check data would have actually had to cross over the curves for the full check data for nonadditivity to hold.

Since differential weighting permits nonadditivity but does not require it, it is possible that nonadditivity might have held for some subjects but not all. To test this, the data from the single subjects were averaged individually over replications and checked for ordinal interactions. The results are given in Table 1. Column 2 gives the number of subjects (out of 41) who were additive for each of the matrices. Column 3 gives the number of subjects who showed the expected "averaging type" of nonadditivity in which the steeper slope of the curve for the "weaker" evidence causes it to cross over the shallower slope of the curve for the "stronger" evidence. (Note that no crossovers were actually predi·ted for the Control matrices. Nevertheless, averaging-type crossovers can occur if the weights for samples vary with diagnosticity.) Column 3 gives the number of subjects who showed forms of nonadditivity other than the expected one.

It is evident from the data that despite an overall tendency to additivity, there were numerous violations of additivity at the single subject level, averaging 2.5 violations per subject out of 6 tests. No subjects had zero violations and only 11 subjects had as few as one. However, the violations were not restricted to the expected form, nor were they restricted to the critical matrices in which differential weighting effects should have been maximized. Thus, Experiment 1 does not provide a clear answer concerning additivity. At the group level, the data satisfy additivity, although this additivity is not particularly well supported by single subject analyses. At the single subject level, there are numerous instances of nonadditivity, although these are often of an unexpected form. With only two replications per data point, it is not possible to tell whether these unexpected forms of nonadditivity are simply noise or whether they reflect some unanticipated but systematic aspect of the judgment process.

Experiment 2 was intended to improve on Experiment 1 in three ways. First, in order to increase the reliability of single subject tests of additivity, replications were increased from two to four. Second, in order to

Table 1

Experiment 1

Single subject tests

of additivity

| Matrix | Additive Responses | Averaging-type Nonadditive responses | Other Nonadditive responses |
|---|---|---|---|
| Control L | 17 | 10 | 14 |
| Critical L1 | 26 | 9 | 6 |
| Critical L2 | 21 | 8 | 12 |
| Control H | 25 | 7 | 9 |
| Critical H1 | 24 | 6 | 11 |
| Critical H2 | 29 | 6 | 6 |
| Totals | 142 | 46 | 58 |

enhance potential weighting differences between full checks and spot checks, a three-stimulus design was used in which one factor comprised single samples and the other factor comprised pairs of samples. Third, in order to provide an estimate of the rate at which nonadditivities occur due to subject error, control matrices were developed in which additivity would be expected to hold for any process in which the final response is simply a weighted average of sample information.

## Experiment 2

### Method

Task and design. The task for Experiment 2 was exactly like that for Experiment 1 except that judgments were based on three samples per trial rather than two. The various stimulus triples were constructed using a two-factor design in which the column levels were all pairs of samples and the row levels were all single samples.

The design can be partitioned into eight 2 x 2 matrices, all of which have both spot checks and full checks on each factor. These are shown in Figure 4. The four matrices shown in the upper panel (two with high values and two with low values) are the Critical matrices in which nonadditivity can occur given particular assignments of weights and scale values. For illustration, the values shown in the figure arise when (a) the scale values are taken to be numerically equal to the sample values and (b) the weights for full and spot checks are, respectively, 3 and 1. Note that the upper pair of critical matrices show crossovers whereas the lower pair show convergence but no crossovers. This difference between matrices reflects the particular "tuning" of weight and scale value magnitudes. When the weights are 2 to 1, none of the four critical matrices has crossovers. At 5 to 1, the lower two critical matrices have crossovers but the upper two do not. At 10 to 1, none of the critical matrices has crossovers.

The four control matrices in the lower panel are of interest because for these matrices violations of additivity cannot occur if the response is any weighted average of sample information. This is because the scale values within each factor are identical. Under this condition it can be shown that the predicted value (i.e., the weighted average) varies as a linear function of the relative weight ratio in a given cell, and such ratios display ordinal independence. Thus, the occurrence of nonadditivity in control matrices will

## CRITICAL MATRICES

| L1 | Full 11<br>Full 11 | Spot 11<br>Spot 11 |
|---|---|---|
| Full 13 | 11.7 | 12.2 |
| Spot 15 | 11.6 | 12.3 |

| H1 | Full 21<br>Full 21 | Spot 21<br>Spot 21 |
|---|---|---|
| Full 19 | 20.3 | 19.8 |
| Spot 17 | 20.4 | 19.7 |

| L2 | Full 11<br>Full 12 | Spot 11<br>Spot 12 |
|---|---|---|
| Full 13 | 12.0 | 12.4 |
| Spot 15 | 12.0 | 12.7 |

| H2 | Full 21<br>Full 20 | Spot 21<br>Spot 10 |
|---|---|---|
| Full 19 | 20.0 | 19.6 |
| Spot 17 | 20.0 | 19.3 |

## CONTROL MATRICES

| L1 | Full 11<br>Full 11 | Spot 11<br>Spot 11 |
|---|---|---|
| Full 14 | 12.0 | 12.8 |
| Spot 14 | 11.4 | 12.0 |

| H1 | Full 21<br>Full 21 | Spot 21<br>Spot 21 |
|---|---|---|
| Full 18 | 20.0 | 19.2 |
| Spot 18 | 20.6 | 20.0 |

| L2 | Full 11<br>Full 12 | Spot 11<br>Spot 12 |
|---|---|---|
| Full 14 | 12.3 | 13.0 |
| Spot 14 | 11.9 | 12.3 |

| H2 | Full 21<br>Full 20 | Spot 21<br>Spot 20 |
|---|---|---|
| Full 18 | 19.7 | 19.0 |
| Spot 18 | 20.1 | 19.7 |

Figure 4. Stimulus design and illustrative data for Experiment 2.

indicate either (a) the base rate of crossovers due to subject unreliability or (b) other factors not accounted for by simply averaging sample information.

Procedure. The procedure for Experiment 2 was essentially the same as that for Experiment 1 except for differences in the stimulus display. The stimulus triples were displayed in a linear format. In every case the single sample from the row factor was inserted between the pair of samples from the column factor. For example, the stimulus triple shown in the lower-left cell of Critical matrix L1 would have been displayed as:

FULL CHECK 11
SPOT CHECK (15)
FULL CHECK 11

As before, subjects were run individually. The session lasted about one hour with a five minute break in the middle. There were 15 trials for practice. These were followed by four replications of the experimental matrices. Stimulus triples were ordered randomly within replication so that no triples from the same matrix appeared in sequence. Counting fillers and anchors, there were 175 trials in all.

Subjects. Subjects were 26 student volunteers from the University of Wisconsin--Madison who participated for pay.

## Results and Discussion

The results of Experiment 2 are in Figure 5. Critical matrices are in the upper panel and control matrices are in the lower panel. Judgments have been scored so that 0 represents certainty that the machine is normal (H10/1000) and 1 represents certainty that the machine is broken (H22/1000). The graphs for the critical matrices have the same general shape as the analogous matrices in Experiment 1. There were significant column effects for all four matrices (with $F(1,25)$ ranging from 20.7 to 56.5) and significant row effects (with $F(1,25)$ from 7.6 to 16.8) for all matrices except L2. Contrary to the case for Experiment 1, however, the interactions for these matrices did not reach statistical significance.

Of the control matrices, all had significant column effects (with $F(1,25)$ ranging from 19.2 to 32.9). Row effects were significant only for L1 and L2 ($F(1,25) = 30.3$ and 13.3). The only significant interaction effect was for L1 ($F(1,25) = 6.6$).

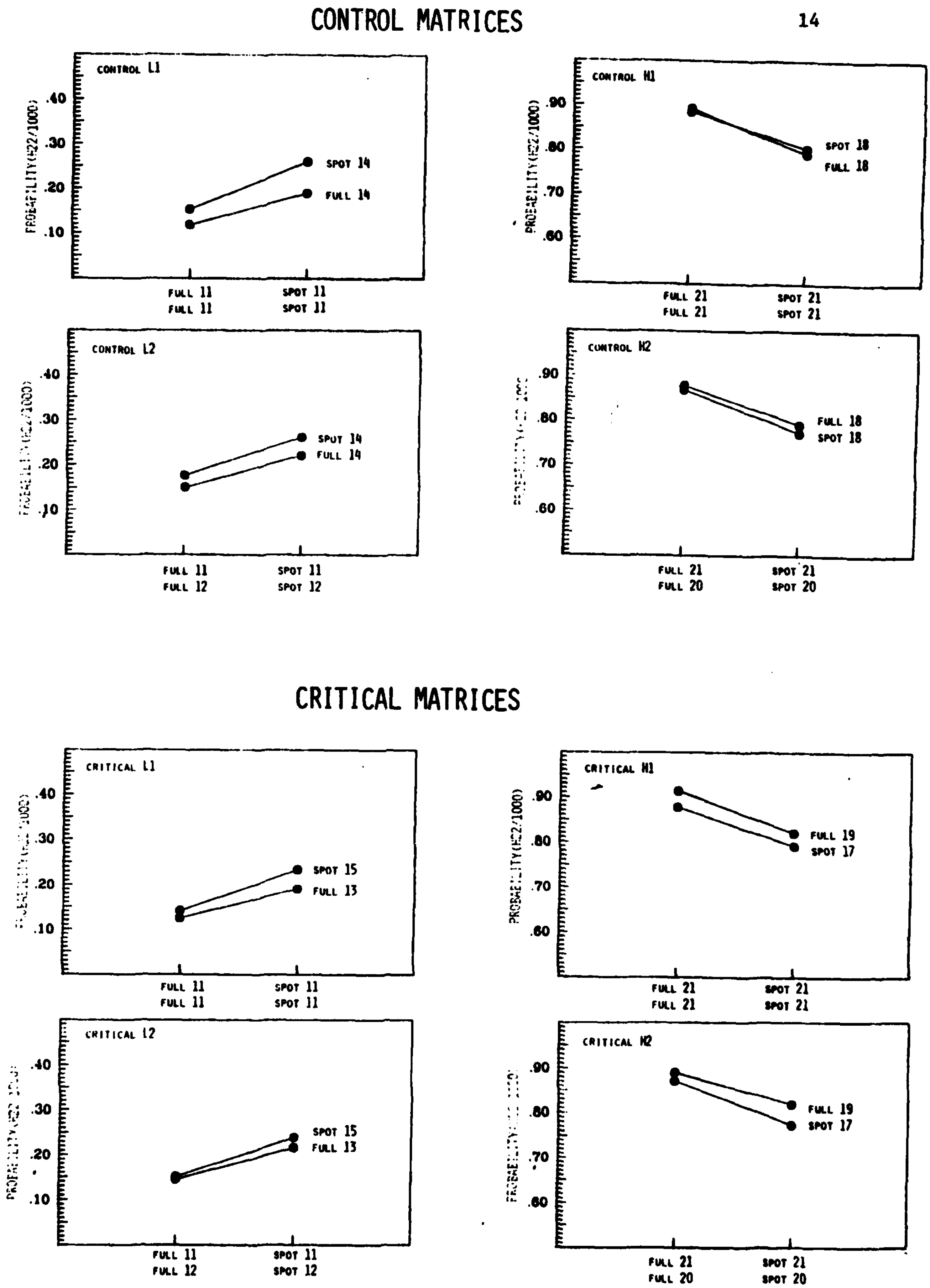



Figure 5. Results from critical and control matrices of Experiment 2.

In general, then, the group data do not violate ordinal additivity. However, tests of additivity at the single subject level revealed about the same level of nonadditivity as was found in Experiment 1. The mean number of nonadditive tests was 3.1 per subject and the median was 4. Only two subjects were additive on all 8 tests. Since these data were based on averages over four replications, it does not appear likely that they are simply noise.

The results of the single subject tests are summarized in Table 2. All together, there were 208 tests, 104 each for critical and control matrices. The proportions of additive tests were virtually identical for both matrix types, 63% and 61%, respectively. As before, reasonably many nonadditivities of the expected averaging type (14%) occurred for critical matrices. However, almost as many of these nonadditivities (11%) occurred for control matrices. Furthermore, numerous nonadditivities of other types occurred for both critical (23%) and control matrices (29%).

Classification of the individual nonadditivities suggested that they were predominately of three types. (In general, four cell means can be arranged in 24 different orders, 8 of which are additive and 16 of which are nonadditive.) First, of course, are the averaging-type nonadditivities in which the slope for the "weaker" evidence is steeper than the slope for the "stronger" evidence. The second type reverses this pattern in that the slope for the "stronger" evidence is the steeper of the two. The third type has a fan shape in which two curves whose slopes differ in sign fan out from a common (or near-common) point. The various numbers of these unexpected types of nonadditivity are given in the fourth and fifth columns of Table 2 and idealizations of all three nonadditive patterns are given in Figure 6.

Inspection of the single subject data suggested that these types of nonadditivities were characteristic of particular subjects and not uniformly distributed among subjects (see breakdown in Table 3). Although there are not sufficient data to test the possibility, it appears that the subjects may have differed qualitatively from one another in the way that they performed the present task. Thus, the additive picture of the inference process revealed in the group data may have been, to some degree at least, created by averaging over subjects who used different judgment strategies. What these strategies were, however, is not certain, although reasonable speculations can be made about two of the patterns. These are given in the next section.

Table 2

Experiment 2

Single subject tests

of additivity

| Matrix | Additive Responses | Averaging-type nonadditivities | Reverse-type nonadditivities | Fan-type nonadditivities | Other nonadditivities |
|---|---|---|---|---|---|
| Critical Matrices | | | | | |
| Low | 28 | 10 | 3 | 7 | 4 |
| High | 37 | 5 | 6 | 3 | 1 |
| Control Matrices | | | | | |
| Low | 32 | 5 | 4 | 10 | 1 |
| High | 31 | 6 | 8 | 5 | 2 |
| Totals | 128 | 26 | 21 | 25 | 8 |

Table 3

Nonadditivity Types By Subject

| SUBJECT TYPE | # of Additive Test | # of Averaging-type Nonadditivities | # of Reverse-type Nonadditivities | # of Fan-type Nonadditivities | # of Other Nonadditivities |
|---|---|---|---|---|---|
| **ADDITIVE** | | | | | |
| S4 | 7 | 1 | | | |
| S5 | 8 | | | | |
| S6 | 6 | | 1 | | 1 |
| S10 | 6 | 1 | 1 | | |
| S11 | 8 | | | | |
| S14 | 7 | 1 | | | |
| S22 | 6 | 1 | | | 1 |
| **AVERAGING** | | | | | |
| S7 | 4 | 4 | | | |
| S18 | 4 | 3 | | | 1 |
| S19 | 4 | 4 | | | |
| S24 | 4 | 4 | | | |
| **REVERSE** | | | | | |
| S12 | 3 | | 4 | 1 | |
| S13 | 3 | 1 | 3 | 1 | |
| S15 | 5 | 1 | 2 | | |
| S17 | 4 | | 3 | 1 | |
| S20 | 5 | 1 | 2 | | |
| **FAN** | | | | | |
| S1 | 4 | | | 4 | |
| S2 | 5 | | | 2 | 1 |
| S8 | 6 | | | 2 | |
| **OTHER AND MIXED** | | | | | |
| S3 | 4 | | | 1 | 3 |
| S9 | 2 | 2 | 1 | 3 | |
| S16 | 4 | 2 | 1 | 1 | |
| S21 | 3 | 2 | 1 | 2 | |
| S23 | 4 | 1 | 1 | 1 | 1 |
| S25 | 4 | | | 2 | 2 |
| S26 | 3 | 2 | 1 | 2 | |

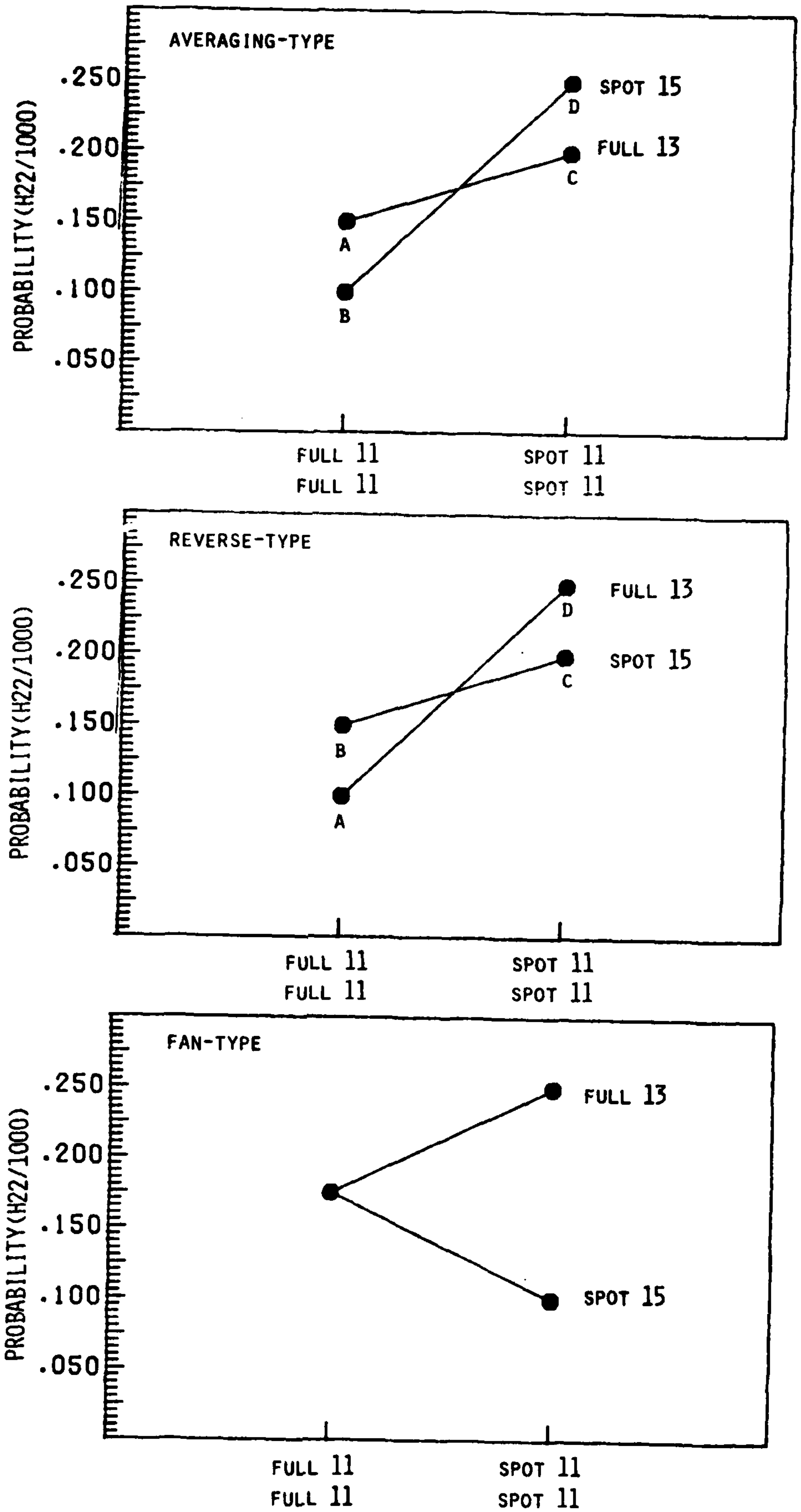



Figure 6. Idealizations of three types of nonadditivity found in Experiment 2.

## General Discussion

The two reported experiments leave considerable ambiguity about whether human inference processes are better characterized as additive or nonadditive. On the one hand, there were numerous instances of nonadditivity in the single subject data, far beyond the number that one would ordinarily find in "good" additive data. On the other hand, the nonadditivities that occurred were often found in unexpected places and with unexpected patterns. Thus, it would be desirable to know whether these nonadditivities make psychological sense.

In this section, I will speculate on why nonadditivity occurred in the control matrices of Experiment 2 as well as in the test matrices, and why reverse-type nonadditivities occurred for at least some subjects instead of averaging-type nonadditivities. Since the psychological theme underlying the present research concerned the role of serial adjustment in judgment, these speculations will be couched in terms of adjustment processes rather than in terms of algebra. I'll begin by summarizing a general procedural theory of algebraic judgment that was presented more completely in Lopes (1982). Then I'll show how this theory applies in the present case.

Figure 7 outlines the major steps that are hypothesized to occur during judgment. In the scanning step the judge quickly assesses the information that has been presented for judgment. On the basis of the initial scan, the judge is hypothesized to select an item to use as an <u>anchor</u> point. This anchor stimulus will generally be chosen because it seems relatively more important than the other available items.

Once an anchor is chosen, it is evaluated relative to the scale of judgment. This "valuation" operation may yield a quantity that serves directly as the initial judgment. For example, in Bayesian tasks such as the present one, subjects may simply anchor their judgment at the value given by the number of rejects in the anchor stimulus (Lopes, 1981). In other cases, however, the anchor point may be a somewhat less extreme compromise between the value of the stimulus information and the value of a neutral "initial impression" (Anderson, 1967).

If after anchoring there are still other items to be considered, the process reiterates with the judge choosing which of the remaining stimuli to consider next. This stimulus is integrated into the judgment by <u>adjusting</u> the initial value according to the new information. It is this step that is

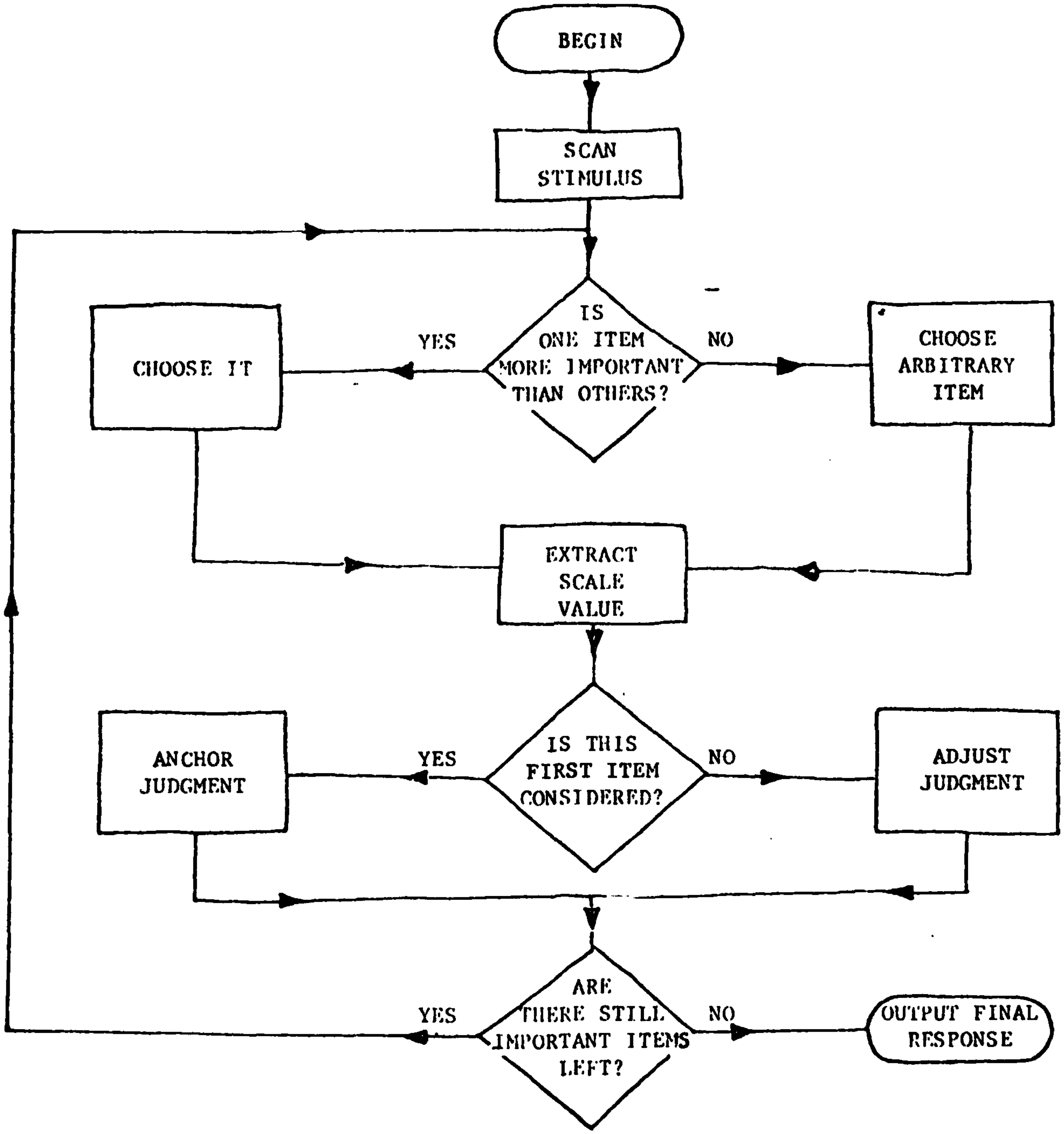



Figure 7. Flow diagram of proposed serial adjustment judgment mechanism.

most critical in determining the algebraic form of the overall judgment. In averaging processes, generally, adjustment is assumed to comprise two stages: (1) location of the new information on the judgment scale relative to the old information, and (2) adjustment of the old judgment value toward the new information to produce a new judgment value that lies between the two. This adjustment process then repeats until all the available information has been used or until the subject judges that enough information has been processed.

Now let us consider how the hypothesized serial adjustment process can lead to nonadditivities. The following five assumptions will be helpful in laying out the general form of the proposed explanation.

1. If a stimulus pair contains both full check information and spot check information, the full check information will be used to provide the judgmental anchor.
2. If a stimulus pair contains all full check information or all spot check information, information that is duplicated will be used to provide the judgmental anchor.
3. If duplicated information is used to provide the judgmental anchor, there will be no further use of the duplicate information in the judgment.

The first two of these assumptions are straightforward and fairly obvious approaches to specifying which stimulus information will be used to anchor the judgment. The third assumption is made for convenience so that problems of specifying whether and how adjustments are made when the "new" information is the same as the "old" information can be avoided. It should be noted, however, that this assumption is not critical mathematically.

4. In situations in which both the anchor stimulus and the adjustment stimulus have the same status (i.e., both full checks or both spot checks), the adjustment will be to a position halfway between the anchor value and the value of the adjustment stimulus.
5. In situations in which the anchor stimulus and the adjustment stimulus have different status (i.e., the anchor is a full check and the adjustment stimulus is a spot check), the adjustment will be to a position one fifth of the way between the anchor value and the value of the adjustment stimulus.

These latter two assumptions merely provide numerical values that can be used to illustrate how differential weighting in the serial adjustment model produces

nonadditivity in a particular data set. Thus, they function only to illustrate relationships that might obtain for a given subject or a given experiment, not relationships that must obtain in general.

Figure 8 shows how, given the five assumptions, the serial adjustment process produces the form of nonadditivity that I have termed "averaging-type". The letters at the end of each of the four "scales" refer to the labels on the four data points of the idealized graph in the top panel of Figure 6. For Point A, the subject anchors (*A) on the value given by the duplicated FULL CHECK 11 samples. Then a single adjustment is made to account for the FULL CHECK 13 sample. Since both the anchor stimulus and the adjustment stimulus are full checks, the final value is 12, halfway between the two values. For Point B the subject anchors at the same point but the adjustment is made to the SPOT CHECK 15 sample. Note that despite the fact that the scale value "15" is further from "11" than is the scale value "13", the final judgment for Point B is actually less than the judgment for Point A due to the proportionally smaller adjustment (1/5 versus 1/2) that occurs when spot check data is integrated into full check data.

For Point C, there are two identical spot checks and a single full check. According to the first assumption, the judgment is anchored at the value for FULL CHECK 13. Following this there are two adjustments made for the SPOT CHECK 11 information. The first of these goes 1/5 of the distance between 13 and 11, yielding an interim result of about 12.6, then the second of these goes 1/5 of the distance between the interim value and 11, leaving a final response of about 12.3. For Point D, the anchor is given by the duplicated SPOT CHECK 11 samples and the adjustment is for the single SPOT CHECK 15. This yields a final value of 13 which, importantly, is larger than the final value for Point C. Thus, the simple serial adjustment model produces a pattern of nonadditivity with B<A<C<D. This is exactly the averaging-type nonadditivity that is described algebraically in Figure 1 and illustrated graphically in the top panel of Figure 6.

A problem, however, arises with respect to the control matrices. Given the set of processing assumptions above, it can easily be shown that there is no weighting scheme applied solely to scale values that can lead to nonadditivities if the scale values within a given factor are the same. Nevertheless, nonadditivities are possible within the serial adjustment model if it is assumed (1) that the subject has a neutral "initial impression" with a non-zero weight and (2) that the effective anchor point in a judgment is actually a weighted average of the initial impression and the stimulus information that is selected by the initial scanning operation. Since there is good reason to believe that initial impressions

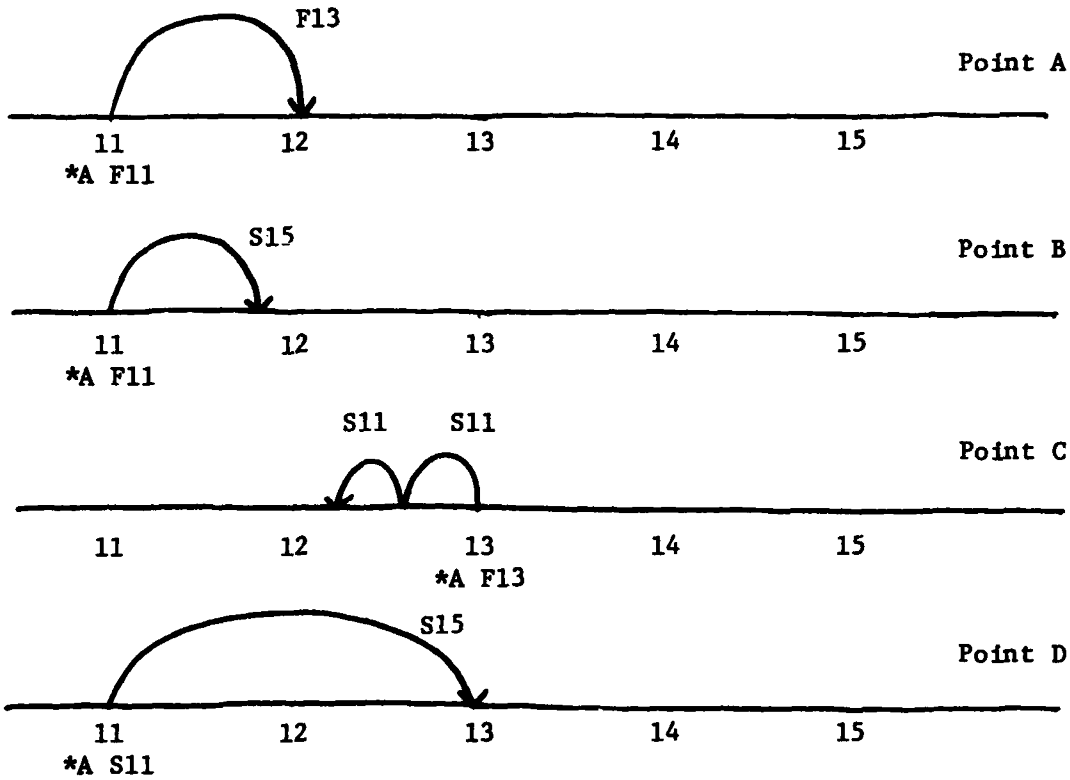



Figure 8. Serial adjustment model applied to critical-type matrix to produce violation of ordinal independence.

commonly influence judgments in this way (cf. Anderson, 1974, 1981), it is useful to see how they might operate in the present task.

The top four scales of Figure 9 show how the serial adjustment model would operate on critical (top panel) and control matrices (bottom panel) if there were a neutral initial impression. For the sake of simplicity, the anchor values for Points A, B, and C in both panels are drawn exactly as they were in Figure 8. This would be approximately correct if the weight for full checks was very much greater than the weight for the initial impression. What is important is that the anchor value for Point D in each panel is drawn as being more neutral (i.e., larger in this case) than is the analogous value for Point D in Figure 8. This difference in position reflects the fact that the relative weight of the neutral initial impression is higher compared to the weight of spot check information than compared to the weight of full check information. Thus, the anchor position for Point D is shifted toward a more neutral position.

Note that the qualitative effect of the initial impression is to produce an identical pattern of nonadditivity in both panels. Thus, there is at least weak support for the hypothesis that a differentially weighted averaging model with an initial impression can account for averaging-type nonadditivities in either critical or control matrices. Can a differentially weighted averaging process also explain the other observed patterns of nonadditivity? For the reverse-type pattern (middle panel of Figure 6) the answer is "no". The pattern can be explained, however, if we assume that some subjects make adjustments that are directionally in accord with the Bayesian model. To see how this might work, consider how such subjects would differ from subjects who follow an averaging-type rule.

The essence of the averaging model is that adjustments to the anchor position are always in the direction of the value of the new information. Thus, if it happens that both pieces of evidence support the same hypothesis and if, for whatever reasons, the evidence that is used to anchor the judgment is the more extreme of the two, then it follows that the adjustment process will be toward neutral. Under Bayes' theorem, however, the adjustment would be toward greater extremity. Thus, subjects whose adjustments are in the Bayesian direction would, for any stimulus pairs in which both stimuli favor the same hypothesis, always adjust from the anchor position toward a more extreme response, no matter in what order the stimuli were processed.

Since previous tests of the serial adjustment model (Lopes, 1980, 1981) have shown that some subjects do make Bayesian-type adjustments when stimuli are presented sequentially, it is not unreasonable to suppose that some subjects in the

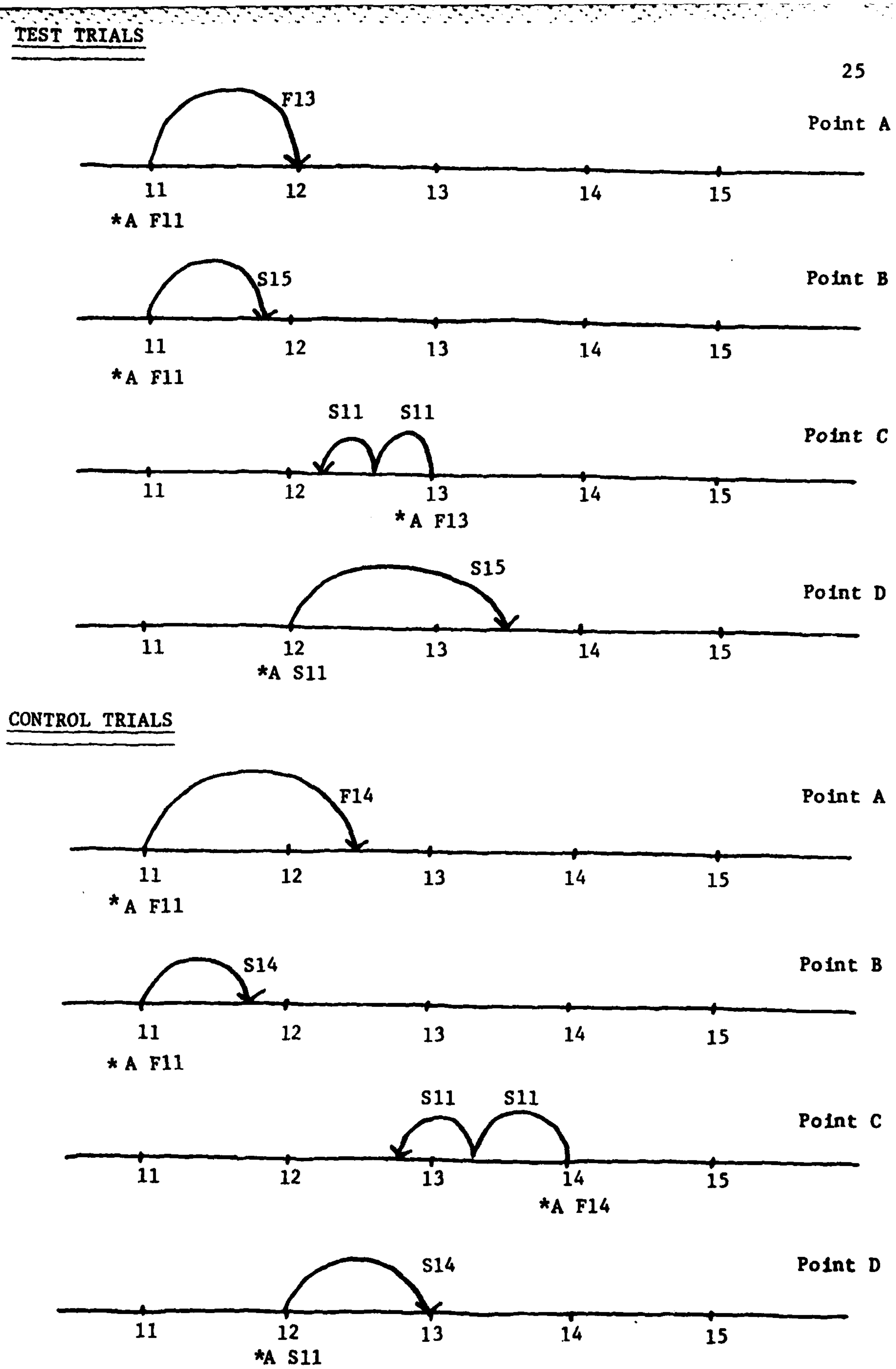
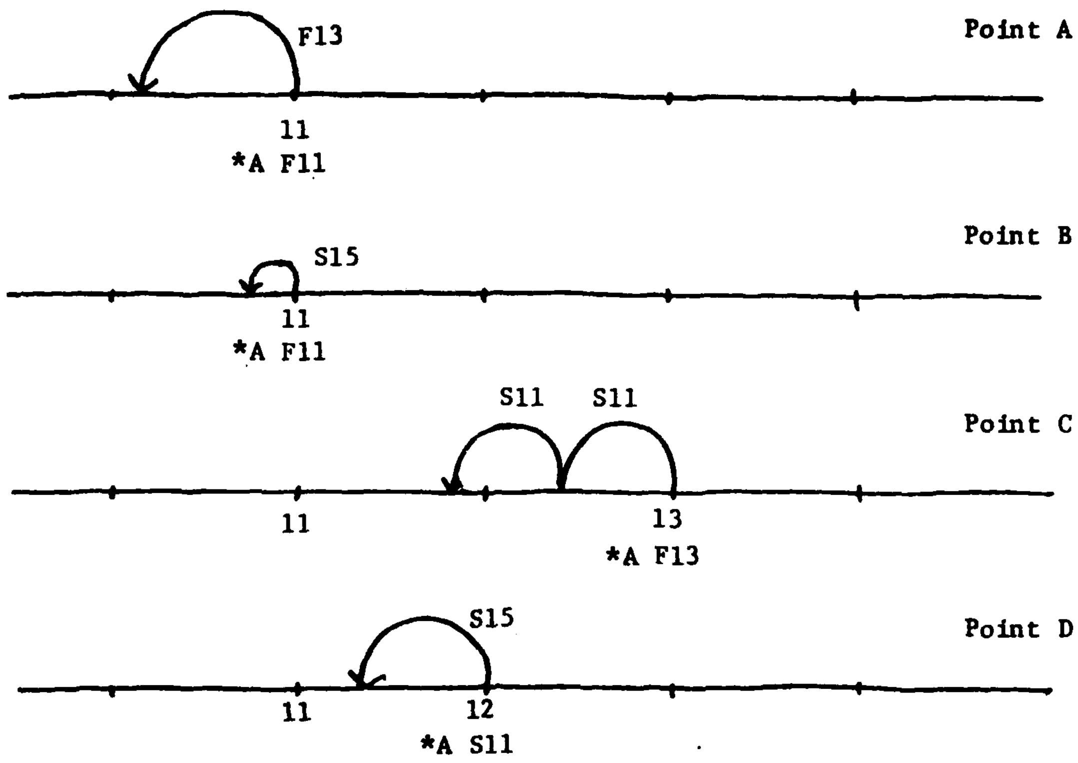



Figure 9. Serial adjustment mechanism with averaging-type adjustments and initial impression.

present experiments might also have made Bayesian-type adjustments. Figure 10 shows how subjects making such adjustments might produce reverse-type nonadditivities. For illustration, consider the upper panel. The judgment labeled Point A is anchored by the FULL CHECK 11 samples and is then adjusted leftward (toward the hypothesis that the machine is normal) to account for the FULL CHECK 13 sample that also favors the machine being normal. The anchor for Point B is the same, but the adjustment is smaller due to the fact that a spot check of 15 favors the machine's being normal considerably less than a full check of 13. Point C is anchored at FULL CHECK 13 and then adjusted leftward twice in accord with the two SPOT CHECK 11 samples. These adjustments are somewhat smaller than the adjustment for the FULL CHECK 13 shown on the top line since they are based on less reliable information than the anchor value that they are adjusting. Lastly, the SPOT CHECK 11 values are used to anchor Point D at "12", which is a less extreme position than is produced by equivalent information from full checks. This is then adjusted to account for the additional information provided by the SPOT CHECK 15 sample. I have shown this adjustment to be somewhat larger than the adjustments for SPOT CHECK 11 on the line above since it is adjusting an anchor stimulus of equal reliability. The final result is that A<B<D<C. Thus, a serial adjustment strategy that differs from the averaging strategy primarily in terms of the direction of adjustments can produce the reverse-type of nonadditivity that was observed to occur in the data.

It appears, then, that a differentially weighted serial adjustment model can account for two of the major kinds of nonadditivity found in the data. However, the prevalence of fan-type nonadditivities in the data is problematical since there does not appear to be any simple way in which a serial adjustment mechanism could account for the negative slope of the curve for spot checks. Thus, although there does appear to be reasonable support for the conclusion that the process people use in Bayesian tasks is more likely to be nonadditive than additive, it is not immediately apparent that the kind of serial adjustment strategy examined here can account for all the data.

TEST TRIALS




CONTROL TRIALS

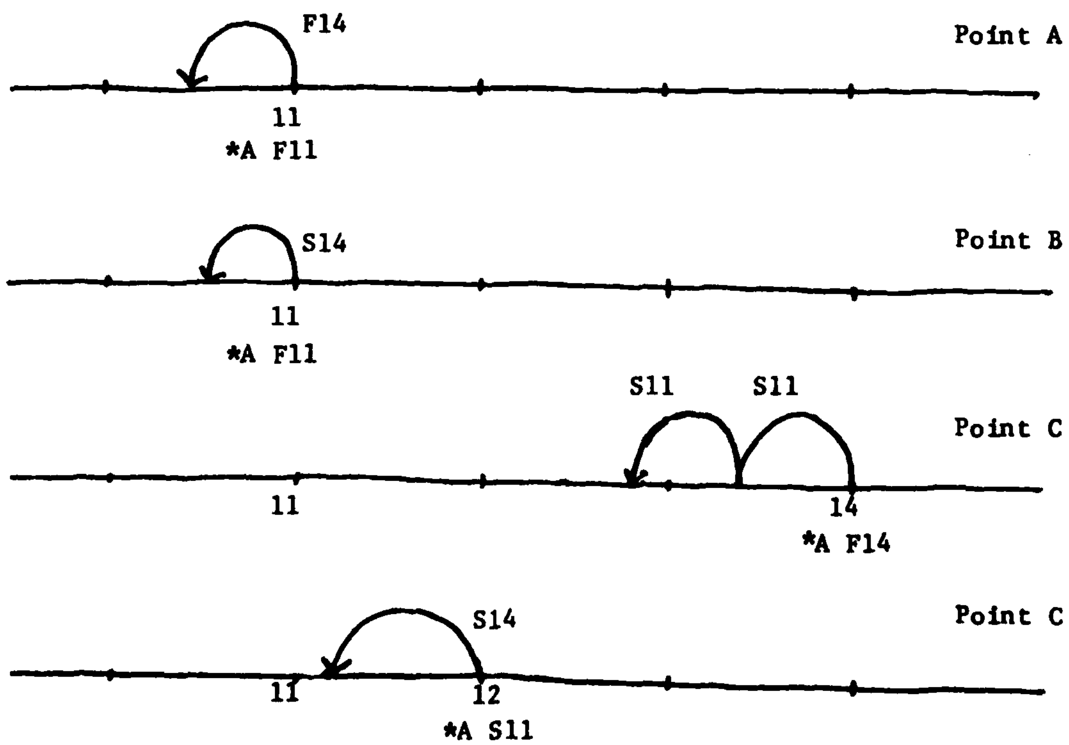



**Figure 10.** Serial adjustment mechanism with Bayesian-type adjustments and initial impression.

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